

Subject: Decision on New Phytologist manuscript NPH-MS-2020-34010
Date: Monday, August 31, 2020 at 2:04:28 PM Eastern Daylight Time
From: Andrea Polle
To: Teixeira, Kristina A.
CC: apolle@gwdg.de, np-centraloffice@lancaster.ac.uk

External Email - Exercise Caution

NPH-MS-2020-34010 Tree height and leaf drought tolerance traits shape growth responses across droughts in a temperate broadleaf forest
by McGregor, Ian; Helcoski, Ryan; Kunert, Norbert; Tepley, Alan; Gonzalez-Akre, Erika; Herrmann, Valentine; Zailaa, Joseph; Stovall, Atticus; Bourg, Norman; McShea, William; Pederson, Neil; Sack, Lawren; Anderson-Teixeira, Kristina

Dear Dr Anderson-Teixeira

Your study has been reviewed by three experts and their comments have been appended at the end of this message. As you can see, most of the comments were very positive but there are still a few points to consider.

Please revise your study accordingly. Please, take great care with this revision since we do not entertain multiple rounds of revision.

We are expecting your paper back within the next four weeks.

Sincerely,

Andrea Polle
Editor, New Phytologist

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In any correspondence regarding this manuscript, please include the manuscript reference number and copy the correspondence to New Phytologist Central Office (np-managinged@lancaster.ac.uk).

Decision: Accept subject to revision

Referee: 1

Comments to the Author

The authors have done a thorough job of addressing my concerns and I found the revised manuscript much improved. Well done!

Referee: 2

Comments to the Author

Thank you very much for sending me this revised version of the ms by McGregor et al. It is a very important study with a clever and appropriate sampling design, and is exceptionally well written. I enjoyed reading it again and find it very valuable. Thanks very much to the authors for tweaking the text here and there and adding detail, which made the ms even more compelling. I had a few reactions that might be useful:

72 One suggestion is that it would be better not to say that larger individuals have xylem of “greater efficiency.” Instead, Red Queen like, conduits widen allowing whole-pathlength resistance to remain constant over much of the height range of trees. Presumably selection favors variants in which leaves maintain their productivity constant insofar as is possible as an individual grows taller, and resistance as constant as possible would contribute to this. So, taller trees don’t have more efficient conduits; they widen in a way that maintains the same whole-pathlength efficiency. Here you could say something like “taller trees have wider conduits, which help maintain constant the resistance that would otherwise increase as trees grow taller.”

392 “Constraint” is always such a vague term in biology (meaning everything from factors opposing selection to selection itself), with every biologist understanding something different. Replacing the term always seems to me to lead to increased clarity. Here, it could be removed entirely “previous findings that it is impossible for trees...”, I think profitably.

395 if taller individuals are exposed to greater evaporative demand, then for a given photosynthetic productivity they would require greater conductance than they would need for a given height if they were sheltered. This would favor even wider conduits, making taller individuals even more vulnerable! You rightly highlight the need to separate height per se and canopy position.

With regard to root mass, this might be useful: <https://nam02.safelinks.protection.outlook.com/?url=https%3A%2F%2Fdoi.org%2F10.1371%2Fjournal.pone.0086550&data=02%7C01%7CTeixeiraK%40si.edu%7C04ca4a8deb534237b5a508d84dd84e13%7C989b5e2a14e44efe93b78cdd5fc5d11c%7C0%7C0%7C637344938706540440&sdata=tiYufg0%2F%2BBuRBThcGeUHuRjVDhWPmbK0xP%2Fb2moH%2F210%3D&reserved=0> seem to show above-below ground biomass isometry.

Thanks again and congratulations to the authors.

Comments to the Author

This is a resubmission of a previously revised manuscript. I have enjoyed reading the manuscript. The paper is well written and the methods are clearly presented. The hypotheses are sound and well framed within the physiological implications of tree height.

The authors have satisfactorily answered most previous comments. For instance, I appreciated the discussion on the two indices R_t and R_{tarima} . I agree that results comparing them are pretty similar (Table S6 vs Table S7) and that authors can use R_t .

Yet, I still believe there are a couple of details that need revision:

1) I still think that adding the same analysis on the two other resilience indices (resilience and recovery) from Lloret et al. (2011) would increase the interest of the manuscript (as the authors acknowledge in line 476). Using DeSoto et al. 2020 as a justification is not enough. There are potential caveats in that paper and even if we accepted the general trends in growth resilience proposed in that study, studies like that of McGregor et al. apply at a different and more detailed scale, where those global relationships might not hold. This adds interest to the present ms, but for that same reason most readers will like to see a similar discussion on the other complementary growth resilience indices together in the same paper.

2) In Line 263 it is stated that both marginal and conditional R^2 are reported, but then I could only find conditional R^2 s in Table S6, S7. Please, report also marginal R^2 in those Tables to show how much variability it is explained by the fixed effects (i.e. the traits and independent variables in Table 3). This is important so we can judge how robust the tested covariates are. I can accept what authors argue in their response ('Tree-ring data are inherently noisy, particularly in more mesic forests. In large part, this is due to strong neighborhood effects, along with variable allocation to ring growth around the circumference of the tree. These factors contribute to the low R^2 , but do not negate the significance of our findings'). However, still I think that, even if (or particularly because) we see many papers today with very low variability explained (see for instance some of the analyses in the previously mentioned DeSoto et al. 2020), we should demand a minimum level of explanatory power in models in ecophysiological papers. Otherwise we cannot assure that inferences are robust enough and relationships worth to be discussed.

Other comments:

- As mentioned on a previous comment, in the manuscript the authors sampled dead and live trees, but then I did not see any further mentioning to this later in the manuscript (lines 175-178 and Table 2). This would merit some discussion and further analysis, how many trees were died and how many trees were alive of those in Table 2?

- Figure S4 and line 333: are differences expressed by different letters correct in Figure S4? Particularly for πtlp (S4d) the boxplot suggests differences (more than, e.g. in b) among different species but then letters are similar (as discussed in line 333).

LMA is not shown in Figure S4, and in Table 2 there are no tests so we cannot judge whether there are differences among species for that leaf trait. Please, add also LMA to Figure S4.

- Line 135: leave turgor 'at' lower

- Line 272: please define $dAIC$. I think it should be better and more robust to stick in all cases to $dAIC > 2$, there are only two covariates selected with < 1 in Table S4.

- Line 312: please, refer to the corresponding Table or Supplement where this statement is supported.

- Line 371-372: actually it is radial growth that is affected; whether that can be extrapolated to the tree level is something that needs to be demonstrated.

- Line 385 vs lines 308-309 and line 338: does that mean that there was a correlation between canopy position and positive R_t ? Can it also be related to moisture sensitivity of different species, or to niche (microenvironment) partitioning among species? (as nicely discussed in lines 420-422). I see from lines 445-450 that sampled FAGR were small, which is coherent with the authors' statement.

Table S5: no need to specify $dAIC > 1$.